this connection, viz. that in Sáukaravijaya of Vidyáranya, the same positions are given for the planets at the birth of Sáukaráchárya, with the exception of the Moon, which is in Árthra, *i.e.* Gemini, 6° 40′ to 20° 0′ of the Hindu zodiac. These positions of the Sun, Moon, Jupiter, and Saturn took place on the 1st of *Mesham*, Kali Yuga 4221, corresponding to March 30, A.D. 1119, without change of the present style.

I am afraid, however, that these dates will hardly suit my Hindu friends, whose devotion to these great personages gives them a sense of "distance" which is best satisfied when expressed in *years!* I give these notes, however, for what they may be worth.

Adyar, Madras. WALTER R. OLD.

NOTE.—According to the Suryasiddhanta rules for computing the longitudes of the planets, I find that Mars was in Capricorn, its "exaltation" Sign, in the month of *Mesham*, K.Y. 1341, as required by the data given for Rama's epoch, its longitude in the Hindu zodiac being Capricornus 13°.—W. R. O.

On the Latent Heat of Steam.

SINCE the invention of M. Berthelot's extremely elegant and simple apparatus, described in his "Mécanique Chimique," vol. i. p. 288, the approximate determination of the latent heat of vaporisation of liquids has become comparatively easy. The exact evaluation of the correction due to the heating of the calorimeter from extraneous sources is, however, a matter of considerable difficulty with the original form of apparatus. The correction is necessarily calculated from data supplied by the thermometric observations made previously to, and after, the actual condensation of the liquid has taken place. For this calculation to be as simple and satisfactory as possible, it is essential that during the whole experiment the temperature of the bodies in the immediate neighbourhood of the calorimeter shall remain approximately constant. In M. Berthelot's method of determination this condition is however not strictly fulfilled. For during the "preliminary period," although the flame is lighted over the calorimeter, the liquid in the flask has not yet begun to boil, so that the radiation to the calorimeter varies, and during the "final period" the flame is extinguished and no further heat reaches the calorimeter from this source. Also during the beginning of the "middle period," a considerable amount of liquid which has been volatilised from the flask at a temperature below its boiling-point, reaches the worm and is there condensed. We therefore modified the apparatus in such a way that the flame was at a constant height and the liquid was boiling *during the whole time of the experiment*, including both the preliminary and final periods. We found that under these circumstances, with a rise of 3° or 4° in ten minutes, the Regnault-Pfaundler correction is perfectly accurate. We propose shortly to publish a complete description of our apparatus, and shall not therefore go into details at present. It differs mainly from that of M. Berthelot, by the insertion in the interior of the boiling flask of a glass valve, which is opened when the rise of the thermometer in the calorimeter has become steady, and closed when sufficient liquid has been condensed in the worm. The vapour during both the preliminary and final periods passes into a reversed condenser.

Our main reason for this communication is to record the somewhat remarkable results obtained with water, and to ask if any of your readers can give information as to any accurate work upon the latent heat of steam published since that of Regnault (*Mémoires de l'Académie des Sciences*, t. 21) in 1847.

We give the results of five experiments (done at pressures differing but little from 760 mm.), which are still subject to certain corrections not exceeding ± 1 unit.

	Wt. of water condensed in grams.	Time of condensation in minutes.	Rise of temp. in calorimeter in deg. C.	Latent heat of steam (L).
(1)	10·122	7½	3·491	525·6
(2)	12·546	15	4·416	524·7
(3)	9·278	8	3·235	526·6
(4)	9·854	7	3·439	525·0
(5)	2·742	6	·991	523·9

It will be noticed that in experiment 5, where the amount of water condensed was purposely reduced, so as to increase as far as possible the experimental error, the result obtained differs but slightly [from the mean. This mean, 525·2 (omitting experiment 5, 525·5) is over 2 per cent. lower than that of Regnault. The thermometer used was one divided into fiftieths of a degree, by Baudin, and was compared with a thermometer calibrated at the International Bureau of Weights and Measures. Every precaution was taken to ensure accuracy of reading.

We have sought for confirmation of our results in the indirect determinations of other observers. If we insert the latest values for the specific volume of steam at 99·6° given by Perot (*Ann. Chim. et Phys.* [6] 13, p. 159) and for the mechanical equivalent of heat by Griffiths[1] (NATURE, vol. xlvii. p. 476) in the thermodynamic formula,

$$L = \frac{T}{J}(S - S')\frac{dp}{dt},$$

we find the number 527·43 for the value of L at 99·60° C.[2] The number given by Regnault for 100° C. is 536·7. We have also selected from the numerous results obtained by Joly (*Proc. Roy. Soc.* vol. xli. p. 358) with his steam calorimeter those relating to silver, which is a substance easy to obtain in a state of purity. If we take the number given by Regnault for the specific heat of silver, we find his own determination of the latent heat of steam confirmed. On the other hand the concordant numbers for the specific heat of silver, given independently by Kopp and Bunsen, lead to a result about 1½ per cent. lower than that of Regnault.

The complete discussion of such results, however, is a matter of great difficulty owing to the uncertainty which prevails with regard to the specific heat of water. We have not as yet succeeded in discovering any constant error capable of explaining the discrepancy between our result and that of Regnault, but further experiments are now in progress.

The question, as need hardly be pointed out, is of considerable practical importance in connection with problems relating to the steam engine.

P. J. HARTOG.
J. A. HARKER.

Physical Laboratory, Owens College, October 19.

Artificial Amœbæ and Protoplasm.

IN No. 1251 of NATURE, Dr. John Berry Haycraft has written a review on Prof. O. Bütschli's investigations of microscopic foams and protoplasm.

The biological parts of the contribution I may leave my colleague, Prof. Butschli, to answer, but as my investigations are also mentioned, and my name several times quoted, though always mis-spelled as "Nuincke," instead of Quincke, I may perhaps be allowed to call attention to the fact that I, not Prof. Bütschli, as the reviewer asserts, was the first who tried to explain the movements of amœbæ and protoplasm by physical laws, by the periodical spreading of a soap solution. In 1879 I explained the voluntary formation of an emulsion observed by Prof. Gad, and the amœboid movements of oil-drops by the periodical spreading of a soap solution upon the common surface of oil and water, and I said "that foam is an emulsion of air instead of oil, and that the durability of foam depended on the same conditions as the durability of an oil emulsion."[3] In a continuation of these investigations I explained in the year 1888 the movements of protoplasm by the same physical principles, making the supposition that it was intermixed with thin oil-films, and in the cells of plants, surrounded by an oil-coat.[4] I therefore believe I was the first to point to the foamy structure of protoplasm, which was later on further investigated by Prof. Bütschli.

Is Dr. John Berry Haycraft acquainted with my investigations, and from whence does he deduce the right of calling them "toys for the physicist"? They form the conclusion of a series of researches on capillarity which I began 37 years ago, and by which I, for the first time, showed that surface-tension is considerably altered by layers of a foreign substance of far less thickness than 1/10 of a light-wave; for the first time, also, the

---

[1] We understand that Mr. Griffiths' number is still subject to a slight correction, but that this does not amount to 1 part in 1000.

[2] $\frac{dp}{dt}$ was calculated from Roche's formula quoted by Hirn, Théorie Mécanique de la Chaleur, *t.* I. p. 325.

[3] G. Quincke, "Ueber Emulsions bildung und den Einfluss der Galle auf die Verdanung" (*Pflüger's Archiv.* 1879. p. 144).

[4] G. Quincke, "Ueber periodische Ausbreitung an Flüssigkeits oberflächen und dadurch hervorgerufene Bewegungserscheinungen" (*Sitzungsber. der Berliner Akad.* 12, 7, 1888. *Wiedem. Ann.* 35, p. 580-642, 1888). "Ueber Protoplasma bewegungen und verwandte Erscheinungen" (*Tageblatt der 62 Versammlung Deutscher Naturforscher und Aerzte*, Heidelberg, 1889, p. 204-7).

© 1893 Nature Publishing Group

sphere of molecular action was measured exactly. A number of physical problems were treated, with which in England Lord Kelvin, the late Prof. Clerk Maxwell, Prof. Reinold, Prof. Rücker, Lord Rayleigh, and others have also occupied themselves. The criticism therefore seems not justified.

I know very well that in Germany several representatives of the descriptive natural sciences do not agree with my views about the structure and the movement of protoplasm. For instance, Prof. Pfeffer[1] reproached me with "having, without deducing my views from admissible foundation on experience in organism, exclusively constructed them by physical experiments, and thereupon demanded, in an unwarranted manner, a peripheric oil-layer for protoplasm."

Here, too, let me remark, that I concluded the existence of this peripheric oil-layer from the globular form of the surface of protoplasm in plasmolysed cells and that I tried for months to find in living cells the characteristic periodic spreading, suspected by me, on the inner side of the hypothetical oil-layer. I have several times observed this spreading and the destruction of the globular form caused thereby. The observations of living cells have led me to fresh physical experiments, which I published in the year 1888, together with my theory of the structure and movement of protoplasm. These theories I have always found corroborated in the continuation of my researches since 1888. My adversaries, on the contrary, have as yet not given a satisfactory physical explanation for the above stated phenomena, the globular form of protoplasm surface and the movements in the vicinity thereof. Up to the present day I believe my views to be correct and irrefuted.

The facts observed and the physical conclusions inferred by me, may appear extraordinary and not very intelligible to another science, but they are none the less correct and useful. Biological science must, well or ill, take into account the fact that the development of the cell and the life of the organic nature depends on masses and layers which cannot be perceived by the microscope alone.

Heidelberg, October 22. GEORG QUINCKE.

Human and Comparative Anatomy at Oxford.

IN the article which appeared in your last number under the above heading, expressions occur which may, I think, lead to misconception as to the position of the department of Human Anatomy. It is of such importance in the interest of scientific medical education that the academical teaching of human anatomy should *not* consist merely in "technical training in anthropotomy," that I cannot allow the statement that the teaching of the subject in Oxford is of this nature to pass without comment. Had the writer of the article in question taken the trouble to inquire of the University lecturer here, or of any of the University professors of human anatomy elsewhere, for instance at Cambridge, Edinburgh or Dublin, or had he consulted any of the leading text-books of the subject, he would have found that its scope is much more extended than he supposes. The misstatement having been made, however unintentionally, must be corrected.

Let me add that the department, which was founded in 1885, was not connected in its origin with the department of Comparative Anatomy, and has had no relation whatever with it since. J. BURDON SANDERSON.

Asymmetrical Frequency Curves.

OWING to the haste with which I looked through the proof of my letter in last week's NATURE (p. 615) two slips escaped me, which I hasten now to correct. The ordinates in the diagram should have been marked $\frac{\alpha p^n}{c}$, $\frac{\alpha n p^{n-1} q}{c}$, $\frac{\alpha n(n-1) p^{n-2} q^2}{1.2.c}$, &c., the factor $\frac{1}{c}$ having been dropped. Further, the value for c should have been

$$c = \frac{\sqrt{2(2\mu_2^2 - \mu_4)\mu_2 + 3\mu_3^2}}{\mu_2 \alpha},$$

my α having been converted into a square power.

The method applied to Dr. Venn's curve fits it with an accuracy only surpassed by the generalised probability curve itself. KARL PEARSON.

University College, October 28.

Telegony.

AS already stated in my previous letter, I have discussed this subject in my recently published "Examination of Weismannism" more fully than in NATURE. If "M. D. H." (NATURE, October 19) will consult the reference given in that letter to this work, he will find the facts to which he directs my attention are there given, together with certain reasons for concluding that they do not materially affect the point in question.

Hyeres, October 26. GEORGE J. ROMANES.

AN ORNITHOLOGICAL RETROSPECT.

DURING the year 1892 there were at least three publications which are of great value to ornithologists, though from somewhat different points of view. They are Prof. St. George Mivart's little work on the "Elements of Ornithology,"[1] Dr. Gadow's "Classification of Birds," published in the Proceedings of the Zoological Society, and Capt. Bendire's "Life-Histories of North American Birds."

To thoroughly appreciate the value of Prof. Mivart's "Elements" one has to be the curator of a museum. Many people, like myself, must have been puzzled by the frequent demand for an elementary, but comprehensive book on birds, such as a man can carry with him on his travels, and many people about to journey abroad have asked me for a small book which would explain to them what certain birds were like. I prophesy that Prof. Mivart's book will make many collectors, and its handy size is one of its best features. There have been many introductory works on ornithology published in this country and America, notably those of Prof. Elliott Coues, but nearly all of them are too bulky, and that is the fault with the most popular works, such as the "Standard Natural History" and Cassell's "Popular Natural History." Commencing in an easy and unconstrained manner, Prof. Mivart in his Introduction leads his pupil on through the various forms of bird-life, his object being not to weight the tyro with too heavy material for study at starting. All the leading Avian types are passed in review and they are illustrated by some admirable woodcuts by Mr. Keulemans, drawn especially for the work. It is, therefore, possible for any one to understand what a particular form of bird is like, the only drawback to this mode of illustration being the impossibility of illustrating the subjects on the same scale, so that some of the smaller forms appear to be larger than they really are in comparison with the bigger birds. This was, however, unavoidable.

Three chapters (pp. 134-234) are devoted to the anatomy and osteology of birds, and a fifth chapter deals with their geological and geographical relations (pp. 235-250). That on the "Classification of Birds" summarises the chief characters for each order, suborder, and family, and lastly there is an enumeration of the genera with the number of species in each. This is of course mainly derived from the British Museum "Catalogue of Birds," and I find that on adding up Prof. Mivart's figures, the number of known species is 11,900. The last time that a computation of the number of birds was made was in 1871, when the late Mr. G. R. Gray finished his "Handlist of Birds," and admitted 11,162 species as then known. This was probably a correct estimate, as I have generally found that the "Handlist" contained about enough false species to counterbalance the number of species described since the work was issued. For similar reasons, Prof. Mivart's estimate of 12,000 species will turn out to be approximately correct, and then by adding the number of species described since his book was published, and others discovered since the issue of the "Catalogue of

---

[1] Pfeffer, "Zur Kenntniss der Plasmahautund der Vacuolen" (*Abhandl. Leipzig, Akad. math. phys. Klasse*, 1890, xvi. p. 279.

[1] St. George Mivart, "Birds: The Elements of Ornithology." 8vo, pp. vi.-329. (London, 1892.)

© 1893 Nature Publishing Group